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ANALYSIS

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TWO SHILLINGS AND SIXPENCE NET

ASIL BLACKWELL · BROAD STREET · OXFORD

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THE EDITOR.

REPORT ON ANALYSIS "PROBLEM" No. 4

"IF A DISTRACTION MAKES ME FORGET MY HEADACHE, DOES IT MAKE MY HEAD STOP ACHING, OR DOES IT ONLY STOP ME FEELING IT ACHING?"

By GILBERT RYLE

THERE were nineteen entries for this competition. Seven of the competitors were clear that it is nonsense to speak of someone having a pain, but not feeling it. Five were clear that it is not nonsense. The remaining seven were not uncompromisingly on either side of the fence.

So I remain satisfied that there does exist a real conceptual tangle here.

The selection of my short list was not easy. Some competitors helped me by spending a lot of their short space in pronouncing generalities about the Nature of a Philosophical Puzzle, or about the Deficiencies of Ordinary Usage and the like. I excluded them without hesitation. They were asked not to talk about doing philosophy, but to do a bit of it.

Some of the competitors unsuspiciously made text book moves with the hospitable notions of *consciousness*, *awareness*, *experiencing*, *knowing* and *feeling*. But the majority realised that these are the very wood piles that hide the wanted niggers.

The competitors whom I have short-listed, as well as one or two others, saw that the concepts of *attending*, *heeding*, *noticing* and *concentrating* are cardinal to the puzzle. But though they operated efficiently with these concepts, they did not operate

enough upon them. Much more, indeed everything, remains to be said, and a good deal requires to be unsaid about this family of concepts. I had hoped to steal an idea or two on this matter from the entries, but have not been lucky.

I put Mr. Hartnack just ahead of the two entries from Western Australia—I hope not too much influenced by the feeling that " them's my sentiments '.

So my list is :—

(1) Mr. Justus Hartnack, Colgate University, Hamilton, N.Y., U.S.A.

(2) Miss Mary McClosky, University of Western Australia.
Mr. John M. Wheeldon, University of Western Australia.

Magdalen College, Oxford,

By Justus Hartnack

To forget a headache is not like forgetting a poem or name because the poem or the name I may recall by trying hard but it does not make sense to say that I was trying hard to remember the forgotten headache. To forget a headache is to stop paying heed to the sensation in my head and to pay heed to something else. We may try to forget a headache as it is a sensation we dislike but we only succeed if an exciting event or thought is able to catch our full attention.

It does not make sense to say that I dislike or mind having a sensation to which I do not pay heed. To dislike or to mind entails heeding. But having a sensation does not entail that I pay heed to it. Thus, if I forget my headache it does not make sense to say that I might still mind having it but sense to say that I still have the sensation. In fact I may shortly be reminded of it, namely as soon as the exciting experience is over and the sensation reconquers my attention.

Moreover, it hardly makes sense to talk about paying heed to or not paying heed to a sensation if there is no sensation to pay heed to. A person who never has headaches does not say that he never pays heed to his headaches or that he never feels them ; he says that his head never aches.

Part of the difference between having a sensation which I am not heeding and a sensation that has gone is this. If I say that my headache is gone I am saying not only that I do not feel it but also that even if I think of it I shall not feel it, whereas a headache not heeded would be felt if it were heeded.

My head may have stopped aching because I took an aspirin but aspirins do not let me forget my headache; their effect is not to make me absent-minded but to relieve me of the unpleasant sensation. Exciting events are not like anodynes; they have no effect upon my sensation but they let me pay heed to them instead of to the sensation.

The person who reports that during the dreadful moments of a near-disaster in his car his head stopped aching is likely either to be taken to report that, as a kind of shock-treatment, the near-disaster momentarily cured his headache; that is, that even if during the dreadful moments he should have thought of his headache he would not have felt it (and this is most likely not what he means); or he will be told that he was mistaken in believing that his head stopped aching; it was only that he was distracted from it, i.e., that he forgot it.

If a distraction makes me forget my headache it does not make my head stop aching; it only stops me feeling it aching.

Colgate University, N.Y., U.S.A.

By Mary A. McCloskey

THERE is an ordinary way of describing such a case which favours each answer. One who no longer felt a headache upon losing himself in an interesting piece of work may either say,

(*a*) " My headache stopped whilst I was absorbed in that job but now it has come back again " or

(*b*) " I was so absorbed in that job that I forgot I had a headache."

Use (*a*) expresses the view that my head stopped aching;

Use (*b*) the view that I stopped feeling the ache.

One might attempt a quick proof in favour of the first view along the following lines. The assertion " I have a headache " is only warranted by my feeling the appropriate ache, and as during the execution of the job I felt no ache, I cannot without lying say " I have a headache ".

There is not even any room for doubt about whether I feel an ache or not. Even in such cases where it is my tooth which is " really " aching, i.e. causing the ache, I cannot say " I thought I had a headache, but I was mistaken—it was toothache." Neither is there any cluster of symptoms which might make me doubtful about what symptoms are likely to follow those I

already have, for " I have a headache " makes few or no predictions. Contrast it, in this respect, with " I have influenza ". There is, moreover, no scope for misdescription because any sort of ache in the head qualifies as a headache, sharp twinges or dull throbbings do equally well.

The criterion for using the sentence " I have a headache " is absurdly simple, and analogies with not so simple cases are misleading. When one no longer hears the ticking of the clock, or where one is too intently looking at the picture to notice the frame, the clock's ticking and the picture frame do not rest with my say so. With a headache esse *is* percipi.

That this outline is not conclusive is shown by considering what can be said for the second alternative. One might say, But, one knows the headache is there even if one is not paying any attention to it and so it is in some way like the clock and the picture frame. One is " dimly " aware of them, and *one could at any time become completely aware of them*. What is so different about saying " I could feel it aching if I stopped concentrating on the job " and " I could hear the clock ticking if I turned my attention to it? "

This might lead us to ask, "Are there then alternative criteria for the use of the sentence ' I have a headache ', feeling an ache in the head, or being able to feel an ache in the head if we attend to it? "

This will not do, but it suggests what has been overlooked. Although " having a headache " does not describe a cluster of symptoms in the way that "having the 'flu'" does, it does entail some duration in time. A single twinge of pain is not enough. Because a headache has some considerable duration, it means that it is proper to say during a whole range of time " I have a headache ", even if at some intervals during that period one may not be feeling anything. In much the same way one can properly say " I love John Smith " without at that moment feeling anything about Smith (although this case is different in many important respects).

Connected with this point about the duration of a headache is the fact that both the given ways of describing the case of one who is distracted from his headache are in common use, and what we say in a particular case depends upon what happens when the distraction ceases. If with the case of someone absorbed in a job, when the job is finished, the headache does not return we would say that his interest in his job stopped the headache. Even if for a period of time, say ten minutes, after

the job was finished, the headache was not felt we would say "It stopped his head aching for a while but it started again". On the other hand if the headache is felt again immediately after the job is finished we would say, "He forgot he had a headache because his attention was fixed on his work and he no longer noticed it", and we would be even more emphatic if during the time he was working on the job whenever he eased up or stopped to take breath he felt the ache, and if while he was working he frowned and frequently passed his hand over his brow.

University of Western Australia.

By JOHN M. WHEELDON

THE answer to this question is not physiological but linguistic. We are not here concerned with what actually happens within my head when I am distracted from my headache but with what we can say has happened with the greatest possible consistency with English usage.

If by "head ache" we only meant those occurrences which attend or cause my feeling pain in the head (e.g. a high temperature or strained optic muscles) we should have to say that I have a "head ache", whenever the requisite number of these are present even though in fact I feel no pain. It is also clear that an enumeration of these disorders still leaves us without the notion of "head ache" for although they may always accompany my head ache, they cause or follow it but are not identical with it. What we must not do is to approach the problem from the point of view of the doctor or psychologist but enquire into the usage of the word "ache" and its relation to feeling.

An examination of situations in which we use the word "ache" shows that we do speak of having aches of which we are not aware, as the following examples show. First, if one goes to sleep with a head ache and awakes with a head ache, one says "I still have my head ache". Second, if one takes an aspirin which relieves it, one says while the aspirin is effective, "My head ache has gone". But when in due course the effect of the aspirin wears off one says "My head ache has come back', rather than "I have another head ache".

The first case points to a belief that one has the head ache all the time one is sleeping, even though one is unable to feel it. The second case is more curious. Here, although one says that the head ache has "gone" and "come back", it will be noticed

that it is none the less regarded as the same head ache—"my head ache". Although it "went" and then "came back" it belonged to me. It was my property but not in my possession. In one sense I "had" it, in another sense I did not.

These two cases suggest the rule. They show that we may correctly say that we "own" or "have" a head ache which we do not feel. This can only mean that a head ache or an aching head *is* something which one may or may not feel. Our awareness of this fact is shown by the use of counter-irritants. These do not get rid of a pain but by directing the attention of the sufferer to an artificial irritation lessen the unpleasantness. Everyone admits that the first ache has not gone, only the attention is elsewhere.

The wording of the question itself supports this analysis. The fact that one can forget a head ache means that the ache is still there although one is not aware of it. Further, it would be nonsense to talk of having an ache if nothing is aching. Therefore to have a head ache cannot be different from having an aching head. This must mean that forgetting my head ache is identical with not feeling my head aching. So the answer must be that the distraction only stops me feeling my head aching but does not stop it aching.

University of Western Australia.

ACHILLES AND THE TORTOISE

By J. M. Hinton *and* C. B. Martin

It seems to emerge from the discussion on the Zenonian puzzles that, though these puzzles may all (as Aristotle thought) depend on a confusion between infinite divisibility and infinite extent, they can nevertheless be produced in a variety of forms which are not all open to one and the same kind of treatment.

It also emerges that some people, such as Professor Max Black,[1] have a logician's interest in the puzzles. They want to expose the invalidity of the sophisms as simply as they can. Others, such as Mr. Thomas[2], seem to have a mathematician's interest in the puzzles in the sense that they want to display some of the mathematical complexities to which reflection on the puzzles might lead.

[1] "Achilles and the Tortoise," Analysis, Vol. 11, No. 5.
[2] "Achilles and the Tortoise," Analysis, Vol. 12, No. 4.

I.

Perhaps it is worth pointing out, for the benefit of readers of the first sort, that in certain forms in which the puzzles constantly recur in the oral tradition, they can be dissolved with the simplest of logical instruments, and without any attempt to elucidate the difference between the ' two infinities '.

These forms are substantially the ones in which the puzzles are presented by Prof. Black and Mr. Richard Taylor,[3] but the wording here is our own :

A. "An object O cannot reach a point s, at a given distance from its point of departure, until it has reached a point r, at ½ that distance. But it cannot reach r until it has reached q, at ¼ the distance, and so on ad infinitum. *Therefore* it can never reach its destination. (Variant : " . . . it can never start.")

B. " Suppose A moves at 10 times the speed of T, and T has a handicap of a given distance. Then, by the time A has covered that distance, T has covered a further 1/10th of it ; and by the time A has covered that 1/10th, T has covered a further 1/100th, and so on ad infinitium. *Therefore* A can never overtake T ".

How can we best deal with these puzzles? As Prof. Black says, it is beside the point to advance premisses from which we can deduce the conclusions we know to be true; and irrelevant, too, even to insist that we do know those conclusions to be true —*do* know that under the stated circumstances the arrow reaches the mark, Achilles overtakes the tortoise.

Yet there is, by the way, a qualification to be made on this second point. It is irrelevant to point out that we know the Zenonian conclusions to be false *unless* the puzzles are being offered as metaphysical proofs of the unreality (logical impossibility), of space, time or motion. In this case we may well reply that some statements about space, time and motion are logically possible, for some are true ; and a pleasant compactness would be given to the argument by producing as examples the contradictories of the conclusions which the sceptic *seems* to be trying to establish. Cf. Moore's *Defence of Common Sense.* The conversation would then take a different turn. The Zenonian might, for instance, make a careful distinction between real and apparent motion in such a way as to admit the logical possibility —and indeed the actuality—of the latter, assigning our examples to that category and still maintaining that *real* motion is logically

[3] " Mr. Wisdom on Temporal Paradoxes," ANALYSIS, Vol. 13, No. 1.

impossible. We could then point out that by 'real motion' we mean what he means by 'apparent motion', and that although in theory we might be induced to change over to his usage of 'real motion', we are not encouraged to do so by his candid intimation that this usage is self-contradictory.

However, for some reason which escapes us, the Zenonian puzzles are not usually presented as proofs of the timelessness, spacelessness and changelessness of reality. They are offered as paradoxes; apparently valid arguments which lead to absurd conclusions. In this case, the problem is indeed "to find out exactly what mistake is made in the argument", to quote Prof. Black. Yet surely to do this we must concentrate on the expressions used in the argument, and show why the premiss-sentences appear to express true propositions which entail the absurd conclusions, and why they do not in fact express such propositions. Instead, Prof. Black tells his story about the machines, which turns out to be intended only to show what is wrong with the *solution* of the Zenonian puzzles offered by Mr. Taylor and others. Even if successful, this would leave the puzzles themselves intact; and the short concluding section, in which Prof. Black returns to the puzzles, seems more like the proclamation of a programme than its fulfilment.

We would like to suggest that the two arguments A and B, and others like them, can be treated as simple fallacies of ambiguity.

They differ slightly, though, from the most familiar cases of such fallacies. In such cases, the crucial sentence may express either a true proposition which does not entail the conclusion, or a false one which does; hence the argument's appearance of cogency, i.e. of having a true premiss which entails the conclusion. In cases A and B, the crucial sentences express either necessary propositions from which the conclusions do not follow, or *commands*, addressed to the moving object or its hypothetical mover; commands which are such that if they are obeyed, the conclusions are true; but which there is no reason for the object or its mover to obey.

In A, the premiss-sentence consists, in principle, of an infinite series of sentences of the form "O cannot reach s until it has reached r", "O cannot reach r until it has reached q", and so on. [Familiar equivalents: "O must reach r before it can reach s", "To reach s, O must first reach r", etc., etc.] Now each of these sentences can express a rather unexciting necessary proposition; perhaps in this context we shall be allowed to say, "can work as a

harmless tautology ". Interpreted in this way, each is an elliptical way of saying, " It's logically impossible that O reach s by a track which lies through r, without first reaching r " : and this comes down to, " It's logically impossible that O pass through r to s but does not reach r before s. ", or " (O passes through r to s) *strictly implies* (O reaches r before s) ", or " *The conjunction of* (O passes through r to s) *and* (O doesn't reach r before s) *is self-contradictory* ". And so it is, indeed. And so is the conjunction of (O passes through q to r) and (O doesn't reach q before r); and so on. But even if we prolong ad infinitum this series of strict-implications, we shall be no nearer a premiss which will entail that O doesn't start or that it is logically impossible for O to start. However long we go on, we shall just be pointing out the self-contradictoriness of a series of conjunctions which nobody wants to assert.

In one interpretation, then, the premiss-sentence of A means the same as a series of strict-implication sentences which express true, and indeed necessarily true propositions. But these necessary truths don't entail the conclusion; the strict-implication sentences don't even look as though they expressed propositions which entail the conclusion.

It is the latent possibility of this interpretation, which accounts for the feeling that the premiss is true. What accounts, then, for the feeling that it entails the conclusion?

Instead of interpreting, " O can't reach s till it has reached r " and its fellows, as strict-implication sentences, we might interpret them as commands. Not, however, in the sense that we could sensibly put forward a series of command-sentences as meaning the same as the premiss-sentence of A. The strict-implication sentences are the only ones it would be sensible to produce by way of giving the *syntactical* meaning of the premiss-sentence. But, for each reader or hearer, the premiss-sentence also has a pragmatic meaning, or ' subjective intension ' ; and the ' command ' interpretation is a part of this meaning.

As we hear or reach each successive sentence in the series " O can't reach s till it has reached r ", " O can't reach r till it has reached q ", and so on, we feel as though we were the unhappy player of some Kafka-esque game in which our efforts to move a piece to its goal are forever frustrated by a series of well-timed and authoritative " Don'ts " .

Perhaps we seize the piece, and are just about to place it on s, the finishing line, when we are told : " No! You can't, may not, must not, put it on point s until you have first put it on

point r ". We shrink back, and begin to lower the piece on to point r, only to have the performance repeated; and so it goes. If the object, or its mover, obeys this infinite series of commands, then indeed it can never reach its destination. It nearly gets there at the first swoop, but thereafter finds itself involved in an unwilling and unsuccessful attempt to get back to its starting-point.

Or perhaps we are just *about* to seize the piece, and move it to s, when the first prohibition rings out. By the time we have located r, and are again about to seize the piece, the second strikes us down; and so on. If the object, or its owner, obeys *this* infinite series of commands, it can never even start, but must wait with infinite patience to learn its first move.[1]

If the object obeys the commands, the conclusion is true; but nothing obliges the object to obey them. Certainly the infinite series of strict-implications provides no grounds for inferring that the object does or must obey these commands. But when things were unclear, the commands seemed to have the authority which really belongs to the harmless strict-implications; they seemed to issue from the Realm of Eternal Law.

Similar considerations apply to B. Each sentence of the form " By the time A gets to p_1, T is at p_2 " *can* be an elliptical way of saying " If A goes at 10 times T's speed and T has a handicap of distance D, then when A has covered D, T has covered $D+\frac{1}{10}D$ ": that is,

" (A goes at 10 times T's speed, etc.) *entails* (when A has covered, etc.)."

Here, as in the other case, once the necessary truth is stated in second-order terms and seen to assert a relation between propositions, the concatenation alone is powerless to revive the feeling which it aroused when first-order terms were sed; the feeling that the conclusion is being demonstrated.

Here the infinite series of commands, addressed to Achilles, requires him to move to p_1 while the tortoise moves to p_2, then move to p_2 while the tortoise moves to p_3, and so on ad infinitum

[1] It must not be thought that the difficulty here rests in the impossibility of completing an infinite series of explicitly expressed commands. The problem could be put in terms of an infinite series of commands of which all but the first few are left implicit, e.g.: ' The following commands are commands of steps that you are to take in travelling from p to s.
' You must move in a discrete series of steps, and you must not step from p to s without first having stepped from p to r, but you must not step from p to r without first having stepped from p to q and you must not step from p to q without first having made steps of smaller length ad infinitum.'
Understanding this infinite series of commands entails understanding that ' obedience ' results in making no step at all, for consistent with the series no information is given as to how large or how small his first step from p is to be.

in ever-diminishing hops. He cannot both obey these commands and win the race; but nothing authorises the commands.

B can often in practice be reduced to A in dialectic. B as above, and then :—

Anti-Zeno : " Yes, and by the time A has gone 10 times the handicap distance, T will have gone once that distance, and hence be 8 times that distance behind Achilles."

Zeno : "Ah, but A can't go 10 times the distance until, etc."

However, this device might be regarded as of doubtful legitimacy. Zeno, instead of replying as above, should accuse Anti-Zeno of merely proffering premisses from which the right conclusions follow, instead of showing what is wrong with the argument.

Unless we are mistaken, then, it is possible to deal with the puzzles in forms A and B without making remarks which are as hard to understand as those of Messrs. Thomas and Grunbaum,[1] or as hard to agree with as Mr. J. O. Wisdom's contention[2] that physical space is not infinitely divisible, and Mr. Taylor's view that it is logically possible to perform an infinite series of acts in a finite time.

In these forms, moreover, the puzzles do not even require any explicit distinction between infinite divisibility and infinite extent to be made.

II

However, there are some forms of the argument which make such a distinction necessary; but even here, it can be made in simple terms. The simplest version of these forms is the following.

C. "An object cannot move from one point to another in a finite time; for it must pass through an infinite number of spaces, and this requires infinite time."

To this it is natural to reply that the phrase " an infinite number of spaces " is ambiguous. Roughly speaking, it may mean

(*a*) An infinitely divisible space, i.e. a space which has no smallest segment

(*b*) A space with a beginning but no end, i.e. one which has no last segment

or

(*c*) A space with neither a beginning nor an end, i.e. one which has no first and no last segment.

[1] " Messrs Black and Taylor on Temporal Paradoxes," Analysis, Vol. 12, No. 4.
[2] "Achilles on a Physical Racecourse," Analysis, Vol. 12, No. 5.

Obviously, these informal distinctions would not do for technical purposes, but they seem adequate to show what is wrong with the Zenonian sophism in the above form. Sense (*c*) does not concern us in this context; and it seems clear that passing through an infinite number of spaces in sense (*a*) does not entail going on forever, while C. rules out sense (*b*).

[To say that any space is an infinite number of spaces in sense (*a*) does not, *pace* Mr. J. O. Wisdom, entail that it is composed of points of *no* magnitude. " For any given segment, there is a smaller " does not entail " There are segments which have no size ". It also does not entail " No segment is adjacent to another ", as has been rather fantastically suggested.]

C. can, however, be revived in a more complex form which seems to cast doubt on the adequacy of this solution :—

D. (i) " Whatever passes from point p_1 to point$_2$, passes through all spaces between p_1 and p_2.

(ii) But among the infinite number of such spaces there is the infinite series which consists of $\frac{1}{2}$ the space between p_1 and p_2, then $\frac{1}{4}$ the space, then $\frac{1}{8}$ the space, and so on ad infinitum.

(iii) Therefore whatever passes from p_1 to p_2 must pass through this infinite series.

(iv) Now if this takes infinite time, *then motion from one point to another in a finite time is impossible.* But if it is logically possible that an object should pass through such a series in a finite time, then let it be supposed that when passing the $\frac{1}{2}$ mark, the object gives off a flash which lasts until it is half-way to the $\frac{3}{4}$ mark, then another flash which lasts until it is half-way to the $\frac{7}{8}$ mark, and so on ad infinitum. *In this case it will have given an infinite number of flashes in the finite time it takes to pass from p_1 to p_2*."

In this form, the argument seems to present us with a dilemma. *Either* motion from one point to another in a finite time is logically impossible, *or* it is logically possible that an infinite number of discrete consecutive events occur in a finite time.

Mr. Richard Taylor and Mr. Watling[1] would say that this poses no problem, since we can happily accept the second alternative. But, surely, this is wrong, and for simpler reasons than those given by Prof. Black.

At the same time, the reasons cannot be *too* simple. It would not, for instance, help to say, " If the object has given just *n*

[1] "The Sum of an Infinite Series," ANALYSIS, Vol. 13.

flashes in a given time, then the *nth* must be the last, hence *n* cannot be infinity". This fails, for Messrs. Taylor and Watling will coldly point out that our premiss holds only for every number *other* than infinity.

The logical impossibility of an infinite number of consecutive flashes in a finite time can be seen by considering the *speed* at which our object is supposed to move. Let us first suppose that it moves at constant speed. Then it will get to the $\frac{1}{2}$ mark in a certain time, to the $\frac{3}{4}$ mark in half that time, to the $\frac{7}{8}$ mark in $\frac{1}{4}$ that time, and so on. Now let us ask rather an odd question, namely: "When will the movement of the object have gone on for twice the time it took to get to the $\frac{1}{2}$ mark?" We may get the unsympathetic answer, "After twice the time it took to get to the half mark, oddly enough. What colour were Solomon's white horses?" But if this answer is correct, so is another, namely:—"When the periods of time taken by its movement since the half-mark, total the period it took to get to the half-mark". But this will never be so. Hence the object will, *per impossible*, never 'get through' the given *period of time*. Nor will it help to go faster and faster! And if it slows down then it can indeed guarantee to reach the end of any given period of time, but it no longer looks as though it could get through an infinity of moves in any such period.

Prof. Black's machines are fundamentally like our object in (D), where the continuous motion of the object is replaced by a series of discrete movements corresponding to the flashes. His machines are, as he says, logical impossibilities; but it is because they would hold back time. (One has a nightmarish vision of their slowing down all possible clocks, but this will never do).

So the notion of an infinite series of acts being accomplished in a finite time is indeed self-contradictory. In Mr. Watling's case, the inclination to say that it is not, seems to spring from two sources. (1) The mathematical dodge of saying that the infinite series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+$etc. ad infinitum adds up to 1 at infinity (Cf. Section III). (2) The fact that we can without self-contradiction say "A *is performing* an infinite series of acts".

It may now be said that we have merely strengthened the dilemma in (D) to "Either motion is impossible, or time is impossible", without showing how it is invalid. And so we have. But its invalidity can be shown by a development of the distinctions used to dissolve (C). Concentrate on sentence (iii). To the two senses of "infinite series of spaces" distinguished under (C), there correspond two senses of "passing through an infinite

series of spaces ". It may mean " passing through a series of spaces which is divisible ad infinitum in the manner described in (ii) " or it may mean " *dividing* a space in that way, approximating to the end as to a limit ". Sentence (iii), under cover of the true assertion that the object must pass through the infinite series in the first sense, smuggles in the suggestion that it must do so in the second.

There are two logically possible alternatives.

(*a*) An object can pass from one point to another (hence through an infinitely divisible space) at constant, decreasing or accelerating speed, continuously or in discrete movements.

(*b*) An object can (in logic, not in fact) move from one point towards another, *at decreasing speed*, in such a way as to go on forever without reaching the second point. It can do this continuously or in discrete movements, in the manner of an instrument dividing the space ad infinitum according to the notorious recipe.

Under cover of the ambiguity introduced in (iii), sentence (iv) sells us the third, logically impossible alternative : here, the object proceeds *at constant or increasing speed* in a series of discrete jumps (or continuously, but simultaneously with a series of discrete occurrences) in the manner of an instrument dividing the space ad infinitum.

It is impossible, logically impossible, that it do this at all ; let alone that it do this in such a way as to reach the end of the space.

Hence (D), too, is fundamentally a fallacy of ambiguity.

III

Mr. Watling says, " Now if Achilles' race can be described by saying that he travels $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$, then Whitehead is correct in arguing that since the sum of this series is 1, and not less than 1, Achilles can finish the race. Achilles can catch the Tortoise because an infinite series has a finite sum ".

First, let us try to get the mathematics clear.

The series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$ is a series which approaches, but does not reach a limit, which in this case is 1. The limit of the infinite series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$ may be defined as the least number such that the sum of the series expanded to any finite number of terms will always be less than that number. The number which is the limit of this particular infinite series may be computed by means of the sum of the numbers in the series plus the last

number of this series added to it wherever the series has been stopped. Thus, the number which is the limit of the infinite series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$ is determined by the sum $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{8}$.

We can now apply this to a non-mathematical case.

Imagine two clocks moving according to a time schedule. Clock A is described as covering $\frac{1}{4}$ the distance in $\frac{1}{4}$ second, $\frac{1}{2}$ in $\frac{1}{2}$ second, $\frac{1}{4}$ in $\frac{1}{4}$ second, and reaching point 1 on the dial at the end of one second. Clock B is described as covering $\frac{1}{2}$ the distance in $\frac{1}{2}$ second, $\frac{3}{4}$ in $\frac{3}{4}$ second, $\frac{7}{8}$ in $\frac{7}{8}$ second, $\frac{15}{16}$ in $\frac{15}{16}$ second and so on *ad infinitum*. It is the infinite series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\ldots$ with the limit 1. Time intervals of size diminishing according to this pattern do not add up to their limit 1 (unit of time). The descriptive statement in terms of the infinite series is self-contradictory as we have shown above. Yet we want to suggest that the account in terms of self-contradictoriness cuts across a very real and more subtle difficulty. That is, what can plague one here is that it looks as if clock B were doing something perfectly possible at the beginning of the series and at some point just before 1 it becomes impossible.

Where does the breakdown occur? The breakdown is logical in kind. It is not the breakdown of a real and valiant clock put to a task beyond its ability. Is it the breakdown of a means of calculating, or a principle of or apparatus for calculation? No, the apparatus is perfectly all right. What is wrong is that it is used where it is logically inappropriate. Thus, saying that clock B cannot reach point 1 in 1 second (or that Achilles cannot reach the tortoise), when described in terms of the infinite series, does not set any limitation upon a real clock or a real runner, though it may appear to do so as a result of confusing the two kinds of breakdown or limitation indicated above. A parallel confusion may exist when a reluctance to endorse such a limitation of real clocks and runners leads to the assertion that the apparatus *must* work where it seems impossible that it should. This is Whitehead's confusion where he talks of the 'fact' that an infinite series can have a finite 'sum' as providing a solution for the Zenonian paradoxes.

A description must now be given of the means of calculating or the apparatus for calculation and what it can do and what it can't.

Assume that a clock hand traverses the successive fractional distances on the dial $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, and so forth in corresponding fractional intervals of time $\frac{1}{2}$ second, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, and so forth.

Assume that any series of fractional distances so traversed

and any series of fractional intervals of time so elapsed can both be represented by a portion of the infinite series of which the first term is $\frac{1}{2}$ proceeding as follows, $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\ldots$ The limit of this series is 1.

In what follows it will be shown that these assumptions rightly interpreted assume nothing unnatural or self-contradictory.

The assumptions allow us to answer the following question. What is the total time elapsed or distance traversed given the number of intervals of time that have elapsed or of distance that have been traversed?

The rule (a consequence of these assumptions) for answering this question is :

If n is the number of intervals in the finite series then the sum of the finite series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$ to $\frac{1}{2}_n$ equals total time elapsed and total distance traversed.

If we are informed that n (number of intervals of time elapsed or distance traversed) is equal to 5 then $\frac{1}{2}_n$ equals $\frac{1}{32}$ and the sum of the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\frac{1}{32}$ is $\frac{31}{32}$. That is, the total time elapsed is $\frac{31}{32}$ of a second and the total distance traversed is $\frac{31}{32}$ of the distance to point 1 on the dial.

What given the length of the last interval of distance traversed or duration of the last interval of time elapsed, is the duration of the next interval of time to elapse or the length of the next interval of distance to be traversed?

The rule for answering this question is :

$\frac{1}{2^{n+1}}$ (the next interval whether of time or distance) equals $\frac{1}{2}_n$ (the last interval whether of time or distance) divided by 2.

If we are informed that $\frac{1}{2}_n$ is equal to $\frac{1}{32}$ then $\frac{1}{2^{n+1}}$ is equal to $\frac{1}{32}$ divided by 2 or $\frac{1}{64}$. That is, the duration of the next interval of time to elapse is $\frac{1}{64}$ of a second and the length of the next interval of distance to be traversed is $\frac{1}{64}$ of the distance to point 1 on the dial.

The assumptions also allow us to answer the following question.

What, given the number of the last interval of time elapsed or distance traversed, is the length of the next interval of distance to be traversed or the duration of the next interval of time to elapse?

The rule for answering this question is :

If n is equal to the number of the last interval of time elapsed or distance traversed then the duration of the next interval of

time to elapse or the length of the next interval of distance to be traversed equals $\frac{1}{2^{n+1}}$.[1]

If we are informed that n (number of the last interval of time or distance) is equal to 4 then $\frac{1}{2^{n+1}}$ equals $\frac{1}{32}$. That is, the duration of the next interval of time to elapse is $\frac{1}{32}$ of a second and the length of the next interval of distance to be traversed is $\frac{1}{32}$ of the distance to point 1 on the dial.

This entire apparatus for calculation is applicable for any interval occurring before one second or point 1 on the dial. That is, can be used to generate descriptions of the movement of clock B at any point on the dial (which can be described according to our assumptions) less than 1 and time before one second. Consistent with this means of calculating there is always a point or instant (which can be described according to our assumptions) nearer to point 1 or one second than any point or instant we may specify, therefore there is no specifiable point or instant before the limits point 1 and one second at which this apparatus becomes unworkable. It does not follow from this that the apparatus is workable at the point or instant which are limits or at points or instants greater than the limits. The apparatus for calculation is limited to providing a means of calculating for any instant or point before the limits the total distance or time traversed or elapsed, the number of intervals of distance or time completed, and the length of the next interval of distance or time to be traversed or to elapse. However, it does not follow that the actual clock is limited to actual movement approaching, but ever frustrated in reaching, those points in time and space which are defined as limits within the scheme of calculation itself. The muddle occurs when one takes the entire apparatus as descriptive of the real clock and its struggle towards point 1 and one second rather than as a mathematical generator of a set of possible descriptions of the clock limited (that is, the generator is limited) in scope. The former is logically inappropriate, for it uses the apparatus to do a job for which it logically cannot be used. Watling and Whitehead are wrong in thinking it can. The latter is logically appropriate, because it uses the apparatus to do a job for which it logically can be used. In the former case the apparatus is used to describe how the clock cannot (despite appearances) reach point 1 or one second or how (according to the Watling, Whitehead misinterpretation of 'sum' of an infinite series) the clock can reach point 1 or one

[1] Thanks are due to Mr. D. Braybrooke and Mr. N. Solomons for their help in formalizing this section.

second. In the latter case the apparatus is used to generate possible descriptions of how the clock reaches any point or instant before but not at or after the limits point 1 or one second.

This final account has been an attempt to show in what way the Zenonian description in terms of an infinite series can be legitimately descriptive and how taken as descriptive in a natural but mistaken way it leads to error (Whitehead and Watling) or paradox (Zeno).

It does not in the least matter whether the Zenonian description is in terms of discrete steps, tasks, efforts, moves or in terms of quite continuous motion as in the above example. The same legitimate descriptive force is available in either case and the same maldescription is tempting.

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ON GOODMAN'S REFUTATION OF SYNONYMY

By LESTER MECKLER

GOODMAN maintains that no two different terms can have the same meaning, and Rudner "extends" this thesis to claim that no two *tokens* can have the same meaning,[1] although this further consequence is disputed by Goodman.[2]

The first striking consequence of Goodman's position, apart from its seeming confutation of "obvious facts" is that if it is true it is undemonstrable. For, according to such a thesis, no chain of reasoning whatever, other than assertions of the form 'A implies A', can be completely reliable, since logical implications, substitutions, replacements, do not yield invariably a truth-value similar to that in the original statement. Thus, we might say, logical conclusions are not strictly *provable*, but are only "indicable".

From this result the following further consequences must be drawn: either (*a*) Goodman's thesis must be "self-evident" and does not require demonstration, or (*b*) it *may* be false despite the most careful "indication" or argument. But on the basis of (*b*) we must also say that it might be *true* despite the unreliability of every would-be demonstration; if so, apart from

[1] Goodman, N. "On Likeness of Meaning," *Analysis* vol. 10. Rudner, R. "A Note on Likeness of Meaning," *Analysis*, vol. 10.
[2] Goodman, N. "On Some Differences About Meaning," *Analysis*, vol. 13

possible " protocol " and " self-evident " statements, we are forced to recognize that there exist both analytic and synthetic statements whose truth or falsity lies beyond any actual or *possible* proof, or else deny meaning to ' truth ' and ' falsity '.

The peculiarity of the situation does not end here—for we have been using the properties of " indicability " to indicate these unforeseen results. Is it permissible to assert the pervasiveness of fallible inference on the basis of fallible inferences? Moreover, can we ever *explicate* properly the concept of " indicability " without at the same time making circular *use* of its properties in such an explication? Clearly, not only are we faced with problems as intricate as those involved in seeking to explicate ' analyticity ' in pre-Goodman logic, but in addition, in *this* case we cannot even guarantee that each one of the steps of the apparent infinite-regress or circularity (whichever it may be), has the *same* precision of " indication " (*e.g.*, degree of " likeness of meaning "). Thus, whatever we *do* say must be built upon a very wobbly structure indeed.

Nor do we have the recourse of " putting eveything on a probability-basis " instead of on a truth-basis, since in pre-Goodman logic ' probability ' has been commonly defined or analyzed in terms of ordinary logical concepts, hence ' indicability ' is prior and fundamental to ' probability ', not *vice versa*.

Whichever way we may attack these puzzles, it seems clear (" indicated ") that on Goodman's thesis we must not only be eternally content with less-than-certainty for all our arguments, but that ' certainty ' itself cannot be explicated.

The havoc wrought in formal manipulations—or at least in *interpreted* derivations, since Goodman restricts the scope of his findings, for some reason, to " natural " languages—should be apparent. I have said that conclusions are " less-than-certain", whatever that might mean. More particularly, consider the following form of derivation: assume $A \equiv B$, and $B \equiv C$. Ordinarily, one would unhesitatingly conclude that $A \equiv C$, by transitivity. Not so in Goodman's world; $A \equiv C$ is not *always* true, since 'A' and ' C ' cannot have the same meaning. Hence, our ordinary notion of " transitivity ", like " certainty ", becomes either (*a*) meaningless, (*b*) some kind of " absolute " (*i.e.*, non-analyzable? non-empirical?) concept, or (*c*) some kind of portmanteau term allowing a disorderly host of shapes and sizes to come under its dominion. We are confronted, then, with the following dilemma: either (*a*) we deny ourselves the use of such undefinable terms as ' provability ', ' transitivity ',

'substitutibility', etc., and reduce ourselves thereby to a state of utter scepticism and incommunicativity, or (*b*) we recklessly continue to use them, though with many misgivings, fearing only that these unlocatable errors we are constantly making by unjustifiably *assuming* analyticity will some day unpredictably leap out and trip us up despite all our precautions.

But let us leave these formal and theoretical problems raised by Goodman's thesis and examine its purport for everyday language-usage. We are told that no two different words have the same meaning. Now, this contention is either (*a*) a blatant and brash denial of the "obvious fact" that synonyms *do* occur, (*b*) an insistence that what *appear* to be synonyms are, in some subtle way, not so, or (*c*) an argument based upon a misuse of the "commonsense" sense of 'meaning'.

Surely Goodman is not content merely to be perverse and to deny the "obvious" without explanation, nor is it his intention to deviate from the accepted sense of 'meaning' in order to analyze it and present to us startling properties. Indeed, we see that his discussion assumes the form of (*b*) (though, as we shall see, he succeeds in "proving" no more than (*a*)): synonyms, Goodman explains, are merely instances of more or less "likeness" of meaning, not of identity in meaning. 'Dog' and 'canine', then, are not alike in meaning, he is ready to insist. But, then, neither are 'dog' and 'cat', according to the same thesis. If Goodman were to admit both of these results without further explanation we could well be sceptical of his analysis, if for no reason other than that his attempt to *account* for the nature of meaning would be utterly incomplete. For we are now confronted not with mere unalikenesses of meaning, but with *differences* of unalikeness. Does Goodman's thesis allow us to explain this?

According to Goodman's criterion of synonymy, words cannot be alike in meaning if they differ in extension, "primary" or "secondary". But Goodman's criterion would fly in the face of the ordinary usage of 'meaning' (which it seeks to help to explicate) if it did not allow us to say, as we often *do* say, that terms coinciding in primary extension (*e.g.*, 'dog' and 'canine') must at least to *that* extent be alike in meaning: the degree of overlap seems to be some sort of measure of degree of kinship in meanings. But if this kind of explanation is admitted by Goodman, then there seems to be no further grounds for excluding other such principles: *i.e.*, to the degree that *kinds* of secondary extensions coincide (*e.g.*, P- and Q-

pictures, -inscriptions, -explanations, and so on) they must also be to *that* extent alike in meaning. On the face of it, these principles would seem to conform also to " common-sense " appraisals of meaning-likenesses : ' dog ' and ' canine ' *are* considered synonyms, whereas ' flea ' and ' insect ' do not coincide in these two respects, and the terms are " less " alike. In short, if Goodman's thesis is not merely that extension-differences entail measuring-differences, but also—for nominalistic reasons—that differences in meaning entail differences in extension, then the thesis must also state—or be compatible with a statement to the effect—that the *degree* (or *kind*) of differences in meaning are a function of the degree (or kind) of differences in extension.

We are therefore confronted with the question whether it *is* possible satisfactorily to account for degrees of unalikeness of meaning, or for " kinds " of meaning, in terms of differences in extension, primary or secondary.

It should be evident that it is unsatisfactory to suppose that these degrees of likeness of meaning can be explained in terms of the *number* of denotata (primary or secondary) shared by the terms in question. For that would make ' germ ' and ' bacterium ' more alike than ' dog ' and ' canine '. Nor is it satisfactory to suppose that we may determine this degree of likeness by computing the proportion of denotata shared as against those unshared by two terms, for this method not only requires us to make an exhaustive counting of the denotata (an impracticable test), but also it fails to take into account that not every *kind* of denotatum, primary or secondary, is of equal indexical importance. ' Human being ' and ' rational creature ' have seemingly identical primary-extensions, and even in many science-fantasy books identical pictures and descriptions ; but a logician will not recognize this closeness of kinship merely because the primary extension-coincidence *happens* to be as it is.

On the other hand, there are difficulties in supposing that a better measure of likeness of meaning is the proportion of *classes* of extensions shared. In the first place, this method presupposes a non-concrete entity, and its use is in contrast to the nominalistic goal of Goodman's whole analysis. In addition, it is plain that the demarcation of one class from another is arbitrary, logically speaking, and that as a result the ratio of classes can be subject to the whim of the computer. But let us even assume that we do have some fixed way of determining beforehand the " relevant " or " important " classes of exten-

sions, in terms of which the proportion of shared *classes* of extensions for any two terms could be determined objectively. Would such a criterion be sufficient ? Certainly it allows us to say that ' unicorn ' and ' centaur ' are more unalike in meaning than (say) ' dog ' and ' canine ' because unicorn- and centaur-pictures, for one, are mutually exclusive, whereas the same relationship does not hold for dog-and canine-pictures. However, this same principle would lead us to believe that ' centaur ' and ' unicorn ' must be closer in meaning to one another than (say) ' dog ' and ' wolf ', since not only do the latter differ in secondary extensions, as do the former, but also in primary extension whereas the first pair share the same (null) class.

In short, not only does use of such a principle presuppose some other criterion for determining " important " and " unimportant " classes of extension, not only does it require us to make use of a non-nominalistic entity (classes) in spite of our nominalistic intentions, but in addition it fails to account for closeness in meaning of empty pairs of terms as against those happening to have primary designation.

Further complications might be brought to bear on the problem if we were to ask, not only how do we distinguish " important " from " unimportant " classes of extensions, for purposes of measuring closeness of meaning, but also whether these classes are to be not only dichotomized, but *weighted.* It is more reasonable to suppose that indices regarding closeness of meaning are not simply-dichtomous—that happening to have the same pictures is less important than happening to have identical verbal descriptions—but if this is so, we will have to account for the weighting principle.

In short, regarding the problem of determining the differences in degree of likeness of meaning, it may be possible to justify some nominalistic set of principles compatible with Goodman's original thesis, which measure such degrees of likeness, but even if this is so, even if these criteria do approximate " commonsense " or " intuitive " appraisals of meaning-likenesses, the *technique* involved does not. It is to be questioned whether such complex philosophical apparati, so different from actual usage, have the same problem in mind.

II

Goodman wishes to save himself from the ridiculous consequence that the meaning of ' cake ' is altered from day to day by the baking and eating of cakes, that ' morning star ' will suddenly differ from ' evening star ' by virtue of the sudden

creation of a new astronomical body, visible each dawn only, by holding that the extension of any term consists in all its actual denotata, past, present or future. Yet, it seems not unreasonable to suppose that we know the meaning of 'cake' even for a *possible* world containing one more cake than does our world. It does not seem reasonable to suppose that if some new-type "cake" thought of by some chef but never actually produced *had* been actually produced, that our original term 'cake' would be inadequate to describe it, that a new term would have to be created. In fact, in actual usage, we do not determine beforehand the extension of a term in order to determine its meaning, but rather we are quite sure of its meaning in the face of possible contenders as designata.

Accordingly, contrary to Goodman's supposition, the meaning of 'cake', or of any other term, does seem independent, or at least partly-independent, of actual denotata, past, present or future. We learn the meanings of terms, often, by observing samples of its accepted designata; but no man ordinarily supposes that the meaning of a term familiar to him changes with the creation of new primary or secondary designata, with the creation even of new *classes* of secondary designata (for example, new ways of writing the term). If the *known* meaning of a term is not always recognized to alter because of the confrontation of the knower by new variants of conventional designata, there seems no grounds for supposing that meanings are strictly *analyzable* in terms of such designata, both known (past and present) and unknown (future). There undoubtedly *is* a connection between meaning and designation, but it becomes more and more dubious whether Goodman has properly analyzed it.

Goodman's thesis, therefore, does in many cases confirm our ordinary feelings that there *are* subtle differences in meanings between words, but it does *not* at all confirm our suspicions as to why, nor as to how *much*. Is the difference between 'pig' and 'porker' only a difference in P- and Q-symbols, or are there, more importantly, "connotations" or "expressive" elements not accounted for by Goodman's principles? For surely the *way* in which 'pig' and 'porker' differ is unlike the way in which 'skillet' differs from 'frying-pan' or 'saucepan'. Yet, on Goodman's view, the same principle accounts for both. Is there, then, a "dimension" of meaning which escapes Goodman's analysis? If so, the latter is incomplete. If not, it remains too general and inaccurate.

III

So far, I have been speaking about the peculiar consequences of Goodman's thesis as though Rudner's " extension " did not apply. In his second article, Goodman contends that " what follows is not that every two word-events differ in meaning, but only that every two word-events *that are not replicas of each other* differ in meaning " (my italics). Whether only this limited consequence, or the broader one of Rudner, follows, we shall see that the entailed difficulties are great indeed.

If it is true that all tokens which are not " replicas " of each other, must differ in meaning, how much alike are ' dog ', ' DOG ', ' *dog* ' and ' dOg ' in meaning? And do any of these compare more favourably than the rest with (say) ' canine '? In fact, can we even say that ' dog ' and ' dog ' are mutual " replicas ", since we know that they do, after all, differ microscopically? It seems that Rudner must be quite right: no two tokens can have the same secondary extension, since each individual token T_1 must differ from some other token T_2 in at least one description of it, a T_1-description which is part of the secondary extension of T_1 and not of T_2.

Yet the peculiar thing about ' dog ' and ' canine ' (and other " like " terms) is that they have in common all their denotata *other* than their individual descriptions (all referring to the single occurrence of that term itself). If this is so, then we seem unable to say that ' dog ' (as an individual occurrence) differs any more from ' canine ' than it does from ' dog ' (another single event). The only way in which this situation can be remedied is to say that various occurrences *like* ' dog '—*i.e.* " replicas " of it—are instances of the " same word ", and that " same words " have all their several extensions in common. By such reasoning alone could we say that the secondary-extension of ' dog ' differs more from 'canine' than it does from another occurrence of ' dog '.

But such a device presupposes some suitable analysis of ' like ' or ' replica ', and there are involved such ticklish questions as showing why ' god ' is not a replica of ' dog ' whereas a handwritten " version " of the latter is, or whereas a sign of the Great Plains Indian hand-language is.

Moreover, would we not have to account here for *degrees* of similarity whereby we rule out some tokens as replicas of other tokens, yet include still others within the scope of our definition? Is a script-version of ' dog ' " more like " the printed word than is the hand-sign for a dog? Is the German adjective ' gut ' a

" replica " of the English noun ' gut ' or not? Perhaps we should want to answer this last question in the negative—but this could be only because we feel that these two usages exhibit different *meanings*—yet, as we can see, this would be a circular procedure for analyzing the difference in meaning between ' gut ' (German) and ' gut ' (English). In short, the implications of the Goodman-Rudner thesis are hopelessly complicated and confusing.

But however much these complications may *motivate* us hastily to right again the upset applecart, it is by no means conclusively shown that such an analysis is in error. After all, true sentences need not be simple sentences, nor " obvious ".

IV

I have discussed some of the consequences and difficulties implied by Goodman's thesis. I wish now to turn to the *structure* of the thesis itself. It is claimed that no two terms are *alike* in meaning if they are *unalike* in either " primary " or " secondary " extension. The " secondary " extension of a term is the extension of the " complexes " of that term. Let us inquire, then, exactly what is the " complex " of a term.

Examples of complexes of ' P ', according to Goodman, are P-pictures, P-diagrams, P-symbols, P-descriptions. On the other hand, P-thoughts are ruled out as proper instances of the secondary extension of ' P ' because thoughts are " mentalistic ", hence not genuine physically-existent entities. Similarly, P-parts and P-blood relatives are rejected because their existence implies the existence of P's, whereas not all ' P '-terms in fact have primary extension (' unicorn ', for example). With these considerations in mind, ' P-complex ' seemingly designates a class of terms constructed by " combining " a ' P ' with some term ' X '.

Now, if ' X ' is a descriptive term, then (barring some " ontological " or merely-grammatical restriction) it may be an adjective or adverb as well as a noun, so that proper instances of ' P-complex ' would not be only ' dog-picture ' and ' unicorn-description ', but ' dragon-ferocious ', ' canary -yellow ', ' siren-sweetness ', and ' runs-quickly '. It is evident that Goodman's analysis suffers from the supposition that only kinds of tokens that need be considered are " noun "-tokens, and that the only kinds of terms that can be added to complete the " complex " are also " nouns " (*e.g.* ' picture ', ' inscription ').

If, on the one hand, we suppose that this restriction is

artificial and without philosophical justification, we find that part of the meaning of the verb ' run ' must also be indicated by ' quickly ', because we can construct the " complex " ' run-quickly ', according to Goodman's rules. This would serve, for example, to clarify the difference in meaning between ' run ' and ' yawn ' because it seems not proper to say ' yawns-quickly ' (although Goodman's rules for construction of complexes does not rule out such a term).

On the other hand, let us suppose that for unexplained reasons, the terms whose meanings are to be accounted for by Goodman's analysis are only nouns, and that only nouns may be added to construct " complexes ". We may still wonder just what the nature of a " complex " is supposed to be.

For the " complex " is plainly not a *logical* conjunction of nouns, nor is it a combination of nouns by means of any other simple logical operation. It might be thought, perhaps, that it is nothing more than a " hyphenating " operation, but this too is inaccurate, unless we are willing to define philosophical concepts in terms of the peculiarities of the English language. For the " P-complex " ' dog-picture ', by way of example, does not translate into hyphenated expression in all other languages. In French its equivalent is ' l'image d'un chien ', in Hebrew it is ' tmunat kelev '. Both the French and the Hebrew expressions indicate that the paradigmatic English phrase is not ' P-X ', but ' X *of* P ', and this accords with the earlier remarks about adjectival P-complexes, although not also of adverbial expressions which seem to have no accepted rendering into the ' of '-form. However we shall see that the adverb is not really an exception, after all.

It seems evident, then, that the analysis of the P-complex depends upon the meaning of ' of ', and that ' of ' is a most decidedly ambiguous term. In ordinary usage, ' of ' can indicate a relationship of legal possession (' a collar of a dog '), of blood-relatedness (' parent of a dog '), of whole-partness (' a tail of a dog '), of logical membership (' the properties of a dog ')—which accounts for the adverbial relationship also, although it is clumsy to say, in ordinary English, that ' the quickness of running ' is an explication of ' run quickly '.

So ambiguous a term of ' of ', clearly, is not very suitable for purposes of philosophical analysis ; moreover, it can be seen that the examples of P-complex offered by Goodman do not exhaust the possibilities. To be sure, by restricting the possibilities of the construction to those terms wherein the existence

of the X does not imply the existence of the P, Goodman seems to have eliminated a great many of the possible kinds of ' of '-relations listed above (such as parts of P's, relatives of P's, possessions of P). But this is so only if we use ' implies ' in a loose sense. For if the condition restricts only *entailed* existence, then we may have ruled out blood-relatives of P as proper P-complexes (such as ' P-father '), but not P-parts—admittedly if the foundation of a house exists, it does not always happen that the whole house exists ; or if a unicorn-foot exists that a whole unicorn must have existed (for we can conceive of an incompleted process of evolution which brought forth only the unicorn-foot but failed to produce the rest of the unicorn).

On the other hand, if we insist that between ' X ' and ' P ' there exists no assertable implication whatever, whether logical *or* probabilistic, then although we have enormously restricted the scope of ' X of P ', we have nonetheless not yet limited it to the examples offered by Goodman. For if ' unicorn-pictures ' and ' unicorn-statues ' are proper instances of the complexes of ' unicorn ', then surely ' unicorn-resemblers ' is too. For unicorn-resemblers includes unicorn-pictures, unicorn-statues, perhaps even horses, mules and other objects " resembling " unicorns, by virtue of their possession of key identical properties, or of key " comparable " properties.

But if ' unicorn-resemblers ' is a proper P-complex construction, then so must be ' unicorn-contrasters ', if " contrasting " properties are selected on the basis of some definable criterion as are " resemblant " ones. Yet the existence of objects in some given kind of contrast with unicorns (say hippopotami) does not entail or probabilistically imply the existence of unicorns.

Thus, not only do we see that ' P-contraster ' properly falls under the heading of ' P-complex ', but that given some suitable criterion of " contrasting ", ' P-contraster ' can refer to perhaps *any* object not already included in the primary extension of ' P ', and thus include it within the secondary extension of that term. In consequence, even if ' P ' and ' Q ' differ in meaning because not all P's are Q's, ' Q ' nonetheless secondarily-designates those very P's which help distinguish the meaning of ' P ' from ' Q '. Hence, every Q-symbol which is not a P, is nonetheless designated by ' P '. In a primary or secondary way, therefore, *every* ' P ' and ' Q ' must designate the same things —namely : *everything*.

This surprising outcome indicates a complete failure on the part of Goodman's analysis to account for meaning-differences

in terms of designation-differences. For if it follows, as it seems to follow, from his thesis and from his loose characterization of 'P-complex', that every two terms must designate the same thing—everything—hence mean the same thing, we have reduced the notion of 'designation' to a status of fatuous characterlessness.

These considerations bring me to raise the elemental question—which might have been asked in any case, even were the consequences of Goodman's thesis not so surprising. And this is: Why should we consider the "secondary" extension of a term as a determinant of its meaning? Indeed, why should it be thought that *actual* extensions, consisting of an aggregate of physical objects, determines the meaning of a term? It can be seen that on the one hand there is no strong reason to suppose that these suppositions correspond to what we "intuitively" feel about what we mean by 'meaning', and also that, on the other, that they are not without their puzzles as supposed explicata of meaning-likenesses.

To be sure, no one would wish to deny the converse implication that meanings determine appropriate extensions; nonetheless, it is by no means obvious that the "nominalistic" desire to avoid "mentalistic" and "metaphysical" entities in a theory of meaning, commits us to analyzing meanings in terms of extensions alone. For example, it is not proven that a "nominalistic" and behaviouristic ("conditioned-stimulus") analysis of meanings are incompatible.

In brief, even if the apparatus of concepts assumed by Goodman had proved convincing as an analysis of meaning, and had no entailed difficulties, it is still permissible to ask on what plausible grounds we are justified in making these assumptions about the distinction between "primary" and "secondary" extension, about the mutual dependence of meaning and actual denotata.

An analysis of the nature of meaning, such as that of Goodman, must not only be internally consistent, genuinely explanatory of actual meaning-usages and -distinctions, but also conformable in its basic concepts to the actual "common-sense" notions it seeks to explicate. It is the claim of this paper that Goodman's analysis fails in the first two of these respects, perhaps also in the last.

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